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THE AMERICAN MINERALOGIST, VOL. 54, MAY–JUNE, 1969

THE PROPORTIONALITY OF QUARTZ IN MYRMEKITE: A REPLY

D. M. RANSOM, *Department of Geophysics and Geochemistry, Australian National University, Canberra* AND EVAN R. PHILLIPS, *Department of Geology, Wollongong University College, Wollongong, Australia.*

We read Shelley's discussion with interest and welcome the opportunity to elaborate further on our rather concise note (Phillips and Ransom, 1968). There are undoubtedly many difficulties inherent in an attempt to measure quantitatively the amount of quartz in myrmekite and the errors involved require consideration.

Our method was based essentially on the measurement of an areal representation (*i.e.* a planar surface) of the quartz and plagioclase in myrmekite colonies. The random selection of gridded squares on random thin-section cuts should have provided a reasonable representation of the areal intergrowths considered, but such a method does not provide for *calculation* of a statistical error. The method that would allow for such a calculation—the point count method—is of doubtful use because possible repeat distances between the quartz and plagioclase are difficult to determine and the chance correlation between, say, a quartz repeat distance and a point count unit would introduce large errors. However, Thomson (1930) compared a method similar to ours with other lineal and areal techniques of microscopic analysis and showed that empirically derived deviations were small, being less than 2 percent.

As far as we are aware our method is not affected by the thickness of a thin section, since we were attempting to measure the relationship between the quartz and plagioclase on a surface without thickness, *e.g.*, the top of a thin-section. As long as random orientations are obtained (and this is unavoidable if a large number of intergrowths are measured) there can be no preference for "cross sections" or "longitudinal sections" of the quartz. Of course, the main problem is to establish if measurement is in fact being made at a surface. Our first step in attempting to ensure this condition was to reject intergrowths with diffuse grain boundaries, the assumption being that the intergrowth did not impinge on the surface on which we were making our measurements. In the final analysis then we were forced to make an observational choice and this, we acknowledge, may be the source of some error. However, we believed that some decision had to be made if we were ever to relate the quartz and plagioclase volumes and the fact that a remarkably close agreement between actual volumes and predicted volumes was obtained suggests that we met with some success.

Shelley states that measurement difficulties are only part of the problem. He claims that a real lack of consistent proportionality exists and hence any measurement is pointless. We suggest that there is no basis for such a statement without quantitative data of the type we have attempted to obtain. We are as yet to see *any* factual data which *negates* the proportionality hypothesis. The two isolated examples which Shelley presents in his Figures 1(a) and (b) may not statistically represent the true nature of the Constant Gneiss. Our results were compiled from 25 myrmekite intergrowths in two rocks, and as such were not biased to fit any particular hypothesis although the results support the proportionality relationship. With respect to Figure 1(a) it is possible that the quartz apparently crowded into the northwest corner of the intergrowth may have been partially derived from the quartz-free region and the quartz may have to be averaged over a larger volume of plagioclase than that in its immediate vicinity. Alternatively, the myrmekite figured may represent merely a chance cut, since all myrmekite intergrowths are three-dimensional phenomena. We note that Shelley records the plagioclase in the myrmekite here as calcic oligoclase. It would be interesting to know the composition of the plagioclase in perthite intergrowths in the same rock and compare this with the plagioclase in the myrmekite.

With regard to Shelley's Figure 1(b), we do not understand the process by which the "later plagioclase growth" supposedly arises. A process involving the development of myrmekite, its corrosion by potash feldspar, and then a later growth of plagioclase seems to us to be unnecessarily complex. We are not convinced that corrosion of the myrmekite is the only explanation for the microstructure illustrated. Further, words such as "corroded" and "altered" do not have much meaning to us in the present context since they call for a conclusion which is not obvious from observation of the photographs.

Shelley points out that perthite is not always quartz-bearing—apparently admitting that some is, *e.g.* the vein perthite illustrated by Spencer (1945), Phillips (1964) and Hubbard (1966). (We do not accept Garg's, 1967, view that vein perthite has to be of replacement origin. Further we note here that the exsolution theory attempts to explain quartz-bearing perthite whereas the original theory of Shelley, 1964, fails to do so.) We understand the explanation of film perthite development envisaged by Hubbard (1967) as follows: the formation of myrmekite (and quartz-bearing vein perthite) is due to an early exsolution process which exhausts the supply of calcium held in an originally homogeneous alkali feldspar. As one of us has suggested previously (Phillips, 1964, p. 57) there would probably be a strong tendency to preferentially remove the smaller divalent calcium from the alkali feldspar lattice so that myrmekite and quartz-bearing vein perthite form first. The exsolution of additional albite rimming the myrmekite (e.g. Phillips, 1964, Plate 4, Fig. 7) takes place later, perhaps over a greater time interval. It is this albite which is represented by the film perthite within the unmixed potash feldspar. This later film perthite does not contain Schwantke's (1909) molecule simply because no calcium is left in the potash feldspar. This argument does not demand that calcium has to be initially distributed irregularly in the potash feldspar. We can find no reason to assume that the calcium necessary for the development of quartz-plagioclase intergrowths is restricted in its source to the immediate vicinity of the intergrowth. Migration over near crystal dimensions is entirely possible and Shelley (1964, p. 43) has in fact taken pains to point this out.

In the last paragraph of his discussion Shelley does not elaborate on how quartz becomes available at the growth site and we can only suppose that he is referring to his original hypothesis that cataclasis forces strained quartz between adjacent plagioclase and potash feldspar grains (Shelley, 1964). This quartz is then trapped by exsolving albite and assumes by recrystallization the cylindrical forms typical of myrmekite. A consideration of the experimental work on calcite and quartz published in the past few years (Griggs *et al.*, 1960; Carter *et al.*, 1964) leads us to reject Shelley's views on the phenomenon of "recrystallization." His belief in the cataclastic origin of the so-called "mortar" microstructures is at variance with the current interpretation of these micro-structures as the result of primary recrystallization. We agree with Shelley that the rate of growth of plagioclase is important in the formation of myrmekite. This has been amply discussed by Ramberg (1962), Carstens (1967) and Hubbard (1967).

The work we have done so far on myrmekite has led us not to support Shelley's (1964) hypothesis but to favor the exsolution theory for myrmekite genesis. For example the granites from New England, N.S.W. (Phillips, 1964) have not suffered cataclasis which is an essential prerequisite for Shelley's theory to be operative. Also we find that the plagioclase in association with quartz is always more calcic than albite while pure rim- and intergranular-albites are free from quartz. Further, the movement of albite into a position previously occupied by quartz means that the potash feldspar supplying the albite must undergo a volume reduction and we have not seen evidence of this.

In conclusion, we acknowledge that the origin of myrmekite is by no means settled, and that it is possible that myrmekite formation is polygenetic. However, we submit that our data presented in Phillips and Ransom (1968) are substantially correct and real. It provides factual information concerning the proportionality of quartz in myrmekite and it tends to substantiate Schwantke's model as proposed in 1909. In addition, considering other more circumstantial evidence in support of the unmixing hypothesis, we suggest that it is by far the most viable at the present time (Mehnert, 1968, p. 199).

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